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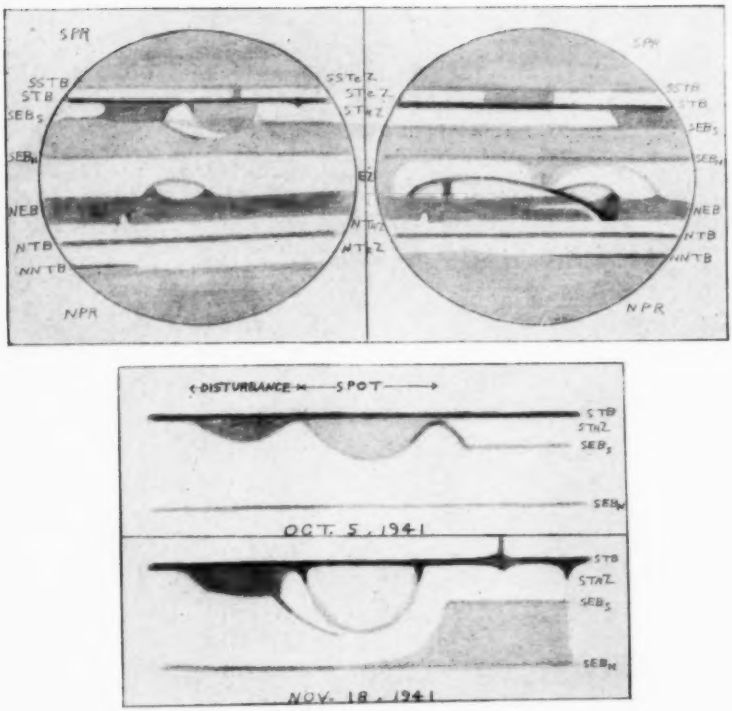
PLATE XV

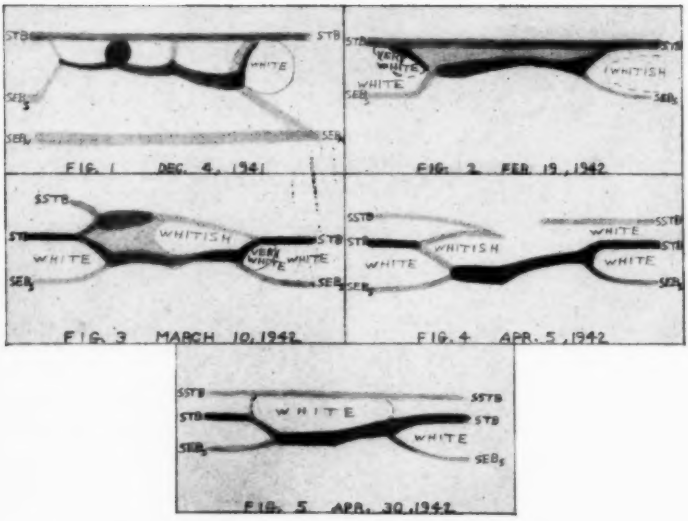


DRAWINGS OF JUPITER BY REESE WITH A 6-INCH REFLECTOR

Above Dec. 22, 1941, 0h 18m Jan. 14, 1942, 1h 16m
CM = $108_1°$, $159_2°$ CM = $177_1°$, $51_2°$

Below Red Spot, STrZ Disturbance, and Vicinity.

PLATE XVI



DRAWINGS OF JUPITER BY HAAS WITH AN 18-INCH REFRACTOR
STrZ Disturbance and Vicinity.

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THE 1941-2 APPARITION OF JUPITER

By WALTER H. HAAS

(With plates XV, XVI)

INTRODUCTION

WHEN Jupiter was at opposition on December 8, 1941, its declination of + 22° made it well placed for northern observers. This paper will report observations carried on by a small group of American amateurs from June 19, 1941, to June 1, 1942. The dates above and all other dates and times in this paper are by Universal Time. The cooperating observers and their usual instruments are listed below. The conflicting presence of Mars in 1941 skies and war-time conditions reduced the quantity of the group's work, the 1942 records being almost wholly by Haas with the 18-inch.

Name	*Station*	*Telescope*	*Remarks*
W. H. Haas	New Waterford, Ohio	6-in. refl.	
"	Upper Darby, Penna.	18-in. refr.	Flower Observatory
"	Springfield, Penna.	8-in. refl.	H. W. Phillipps' Telescope
H. M. Johnson	Des Moines, Iowa	8-in. refl.	
"	Rochester, N.Y.	5-in. refl.	R. T. Smith's telescope
"	Rochester, N.Y.	7-in. refl.	F. R. Vaughn's telescope
E. Reese	Uniontown, Penna.	6-in. refl.	
F. R. Vaughn	Rochester, N.Y.	5-in. refl.	R. T. Smith's telescope
"	Rochester, N.Y.	7-in. refl.	

Such subjects as the proper methods of observing Jupiter, the nomenclature of the belts and zones (note Plate XV), and the two systems of longitude used have been treated elsewhere.[1,2]

This paper will use these abbreviations: *S* for south, *N* for north, λ for Jovian longitude, *CM* for central meridian of longitude, *p* for preceding (direction of decreasing longitude), *f* for following (direction of increasing longitude), *B* for belt or band, *Z* for zone, *R* for region, *E* for equatorial, *Tr* for tropical, *Te* or *T* for temperate, and *P* for polar.

Plate XV (above) may be regarded as typifying the general appearance of the planet in 1941-2.

The Belts and Their Intensities

Data on belt-intensities in 1941-2 fall under three heads:

1. Haas made 81 sets of estimates of the *intensities* of the belts on an arbitrary numerical scale between June 19 and May 10.

2. Johnson made 22 sets of estimates of the relative *conspicuousnesses* of the belts between June 30 and April 26, chiefly before November 21. He used conspicuousness as an integrated effect of intensity and width.

3. All observers recorded notes upon the strength of belts and changes in their appearance.

The *NEB* was at all times much the most conspicuous belt. Its *average* intensity was comparable to that of the most intense other belts; and its greater breadth assured its dominance, Haas once calling it "at least three times as wide as any other belt." The intensity of the *NEB* was fairly constant during the apparition, though perhaps affected by some short-period changes. Johnson described its edges as narrow definite belt-like darkenings, fairly continuous across the planet but concentrated in places. Haas emphasized the complexity of structure in this belt and the lack of uniformity of its shading and saw dark condensations at its edges. He remarked similar intricacies less well in other belts.

The *STB* usually ranked second in conspicuousness, being sometimes equalled or surpassed by the *NTB*. The *STB* was similar to the *NEB* in average intensity but was narrow. The *STB* grew darker near opposition. On December 22 near $CM_2275°$ Reese saw the belt split into two components by a narrow white strip along most of its length. In July and August Johnson and Haas found the *STB* unusually light near $\lambda_2150°$, and on April 13

and 16 Haas found it lightened in the longitudes of the Red Spot ($\lambda_2$158° to 187° at opposition). Reese on September 28 and Johnson on October 2 observed that this belt was heavier at the *f* end of the Spot. Intensity-variations in the *STB* near a strong *STrZ* Disturbance will be discussed later.

Lighter than in 1940-1[3], the *NTB* was second or third in relative conspicuousness, usually the latter. It was narrow, and it was usually less dark than the *STB*. Though the *NTB* did not change progressively in intensity during 1941-2, it frequently differed greatly in strength in different longitudes and sometimes showed fairly rapid changes. Examples follow. It was much darker *f* about $\lambda_2$190° on March 13 and perhaps also on March 23 and 25 than *p* that longitude, a difference gone by April 2. A section near $\lambda_2$300° darkened between August 23 and September 2 but was fainter again in October and November.

Johnson usually ranked the SEB_n fourth in relative conspicuousness, occasionally fifth. It was so light and diffuse that it would have ranked lower if it had not been broader than most other belts. This belt was probably fainter in August and September than in July; it then apparently darkened again in October and November. Possible subsequent minor changes are uncertain. More rapid changes in intensity also occurred; for example, near $\lambda_1$330° the belt grew darker between December 4 and 8 and lighter between January 30 and February 2.

Sometimes invisible when the belt-system was not well displayed, the *SSTB* usually ranked fifth, occasionally fourth or sixth. It may have been wider than most other belts and correspondingly lacking in intensity. It surely averaged more conspicuous than in 1940-1[3]. It exhibited no progressive changes in 1941-2 but showed some variations and was often stronger in some longitudes than in others. On January 14 Reese drew the *SSTB* to resume at the south edge of the *STB* near $\lambda_2$180° and *f* that position to deflect southward to its normal latitude. The behaviour near the *STrZ* Disturbance will be described below.

The *NNTB* ranked from fifth to eighth when seen, averaging about sixth. It was at times vague enough to be quite invisible, even in the 18-inch refractor. In September-January Reese sometimes called the *NNTB darker* than the *NTB;* the latter was

probably always more *conspicuous* because wider. There was again no trend toward either increasing or decreasing darkness during the apparition. The *NNTB* near $\lambda_2 120°$ was fainter to Hass on April 16 than on April 6. The belt was darker *f* $\lambda_2 50°$ during much, perhaps all, of the apparition than *p* that longitude and showed other less durable distributions of darkness.

The SEB_s came from fourth to eighth in relative conspicuousness, averaging near sixth; it was sometimes invisible, once when eight belts were recorded. This belt *averaged* about as conspicuous as in 1940-1.[3] By July the fading which began in the autumn of 1939[2,3] had ended, and Johnson's estimates indicate greater strength in October-November than in August-September. On August 3 Johnson saw a remarkable appearance: the SEB_s resumed at the *f* end of an enlargement ($\lambda_2 212°$) on the north edge of the *STB* and *f* that feature bent northward to its usual latitude. The effects of the Red Spot Hollow and the *STrZ* Disturbance on the SEB_s will be reported below.

The *NNNTB* was ranked from fourth to eighth when observed but was often invisible, even with the 18-inch and even when seven or eight belts were recorded. Johnson frequently noted that the edge of the shaded *NPR* was not defined well enough for a belt. The *NNNTB* was fainter than during the latter half of the 1940-1 apparition[3] but did not appear to change further in 1941-2. Relatively strong sections lay near $\lambda_2 25°$ on September 14 and 22 and *f* $\lambda_2 180°$ in late March and early April.

An *SSSSTB* was visible only in April and May; it probably existed only from $\lambda_2 80°$ to $\lambda_2 250°$. It was fairly dark, darker than the *SSTB*, and bounded the shaded *SPR* when seen. A computation based on a visual estimate by Haas, Peek's measured latitude of the *STB*,[4] and suitable equations[5,6] gives the latitude of the north edge of the *SSSSTB* to be about $-6°$ and appears to confirm its identity.

Even more delicate than in 1940-1,[3] the *EB* was faintly visible only three times.

With one possible exception the *SSSTB* was missing, having vanished since 1940-1.[3] The *SPR* usually extended northward almost to the *SSTB* but had no belt for an edge.

The "dark belt or streak in the middle of the *NPR*" during the

preceding apparition[3] was perhaps recorded when Johnson on November 16 noted an *NNNNTB*.

Other General Appearances

On October 26 Vaughn and Johnson agreed that the extreme north and south limbs were whitened near $\lambda_2 250°$. On July 21 Johnson described the polar shadings as comparatively light near $\lambda_2 45°$, the south one being somewhat the darker.

Johnson recorded a narrow white *NNNTeZ* in the *NPR* on October 26 near $\lambda_2 190°$. The *NPR* sometimes extended to the latitude of the *NNNTB*, sometimes to that of the *NNTB*. Haas observed the former condition near $\lambda_2 230°$ from March 23 to April 6 and *f* $\lambda_2 130°$ on April 15, though not on March 27. There thus resulted a wide *NTeZ-NNTeZ*, sometimes with the dividing *NNTB* faintly present in it. Where seen alone in late March and early April the *NTeZ* was narrow.

The *NTrZ* was narrow, probably throughout the whole apparition. Haas in April estimated it to be about two-fifths as wide as the *NEB*.

The *EZ* was notably white; though second to the *STrZ* before opposition, it was clearly the brightest zone on the planet in March and April. The space between the *SEB* components was abnormally wide. On August 3 Johnson depicted this space dusky *f* the resumption of the SEB_s near $\lambda_2 212°$, and Reese on November 27 compatibly spoke of a grayish shading between the components *f* the Red Spot. On December 10 Reese thought that this space was getting darker, and his drawings on December 22 and January 14 show a perhaps planet-wide faint duskiness. Haas on May 10 and 12 found this space so darkened that the *re* appeared to be a single very broad *SEB*, an aspect perhaps already visible to Johnson on April 26.

As implied above, the *STrZ* was narrow and notably bright.

When the *SSSSTB* existed it was accompanied by a very wide combined *SSTeZ-SSSTeZ*, and the *SPR* was correspondingly reduced in size.

Colours

Available data on colours in 1941-2 consist of 83 estimates by Haas with a 6-inch reflector between July 29 and September 7, 63

estimates by Johnson with the three telescopes he used largely between July 23 and November 19, 29 estimates by Reese between September 26 and January 14, and 13 estimates by Vaughn chiefly with a 7-inch reflector on November 28. Almost all the estimates were made in a clear and dark sky. Haas alone used Eastman Kodak Company Wratten Filters 25 (red), 58 (green), and 47 (blue) as an aid in discerning colours.

The shaded *NPR* looked very slightly bluish to Haas in August and early September, probably about as blue as in September, 1940.[3] He found it more blue near $\lambda_2 285°$ than elsewhere. Vaughn confirmed *faint* bluishness on November 28. Johnson called the *NPR* greenish on August 6 and purplish on October 2.

The *NTB* was less red than in 1940-1, perhaps because fainter.[3] Johnson found it an admixture of red and brown from early August to late November with a trend toward brownness. It may be confirmatory that Haas usually recorded copper-red in August and early September and that Reese observed chiefly brown in September-December. Perhaps the colour of the belt was primarily gray between $\lambda_2 190°$ and $\lambda_2 310°$ for about two weeks in August. Vaughn once recorded purplish blue.

The *NTrZ* once appeared grayish-yellow to Johnson and once looked tawny to Vaughn.

The *NEB* to Haas showed a redness of varying tone. Johnson described it as a changing admixture of red and brown, most frequently as "reddish brown". Vaughn's estimate confirms Johnson. Reese recorded either golden-brown or ruddy. The colour of the *NEB* did not change progressively, but near $CM_1 215°$ and $CM_2 245°$ Haas found a variation from "dark copper-red" on August 4 to "blood-red" on August 6.

The *EZ* was primarily white. The hue was brighter than in 1940-1[3] and perhaps grew purer as the apparition advanced.

Less red than in 1940-1[3] but indeed almost too faint to show colour, the SEB_n looked copperish-gray to Haas in August, gray to Reese in September and November, bluish to Johnson near November 1, and lavender to Vaughn on November 28. Any changes in colour are uncertain.

On November 27 and December 10 Reese called the space between the *SEB* components gray.

The rarely observed *SEB*$_s$ looked grayish to Reese, lavender to Vaughn, and purplish or bluish to Johnson.

The *STrZ* was white.

The Red Spot showed a pronounced yellow-ochre colour to Reese whenever he observed it. The British observer Hargreaves also saw this colour.[4]

The *STrZ* Disturbance looked gray to Reese on December 19 and January 14.

From late July to late November Johnson noted a mixture of red and brown in the *STB* with a trend toward increasing brownness. This trend may be confirmed by the facts that Haas observed primarily copper-red in August and early September (though with some grayness near August 1) and that Reese observed chiefly brown or gray from late September to mid-January. Vaughn remarked a possible slight reddishness on November 28. Haas' estimates indicate increasing redness during the month of August; Johnson's simultaneous work is too small in amount to confirm or refute this possibility. Johnson's work and Haas' work each exhibits short-period changes in the colour of this belt like those reported for many regions during the two preceding apparitions[2,3] and based upon more abundant colour-data than are available for 1941-2.

The shaded *SPR* looked slightly bluish to Haas in August and early September; it was more blue than it had been in 1940-1[3] or than the *NPR* was in 1941-2. Vaughn recorded a definite orange in the *SPR* on November 28. Johnson noted purple on August 6 and gray or greenish gray on October 2.

Red Spot and Hollow

The observers all agree that the Red Spot was very faint and delicate and that it was lighter than during the preceding apparition.[3] Reese saw it best, and his drawings on Plate XV illustrate its appearance. The dark *p* and *f* edges were sometimes the only parts of the Spot visible to Haas. Johnson and Vaughn observed this mark to be darker and more distinct at its *f* end than at its *p* end, and Haas probably saw chiefly the *f* part while using his six-inch reflector. Johnson on November 19 drew a rift between

the Spot and the *STB*, and Vaughn somewhat compatibly depicted the Spot lightening toward the belt. Johnson and Vaughn saw the *STB* indented south of this feature, and some of Reese's drawings are confirmatory.

The Red Spot was presumably more intense in June-August, when Haas saw it faintly with his six-inch, than in December-April, when he could not perceive it with an 18-inch telescope (even with splendid views at times). Possible additional variations in strength in the spring of 1942 are uncertain.

Vaughn has offered an explanation of Haas' failure to see the Red Spot while Reese was observing it with a much smaller telescope chiefly interesting for its possible general applicability to visual planetary studies. Vaughn opines that if Haas' eye is more red-sensitive than most eyes (as is suggested by some of the group's colour-observations), then for him the yellow-ochre Spot would offer less contrast with its background than for Reese with his comparatively blue-sensitive eye. Vaughn further points out that a red-sensitive eye might see the canals of Mars with comparative ease if these bands share the green-blue hue of the Martian *maria*.

The Red Spot Hollow was extremely difficult, even more elusive than during the preceding apparition.[3] Two transits by Johnson suggest for the Hollow a length of fully 48 degrees. The only reference to its tone is a "dull white" recorded by Johnson on December 18. The SEB_s was again either absent or deflected northward in the longitudes of the Hollow. There is some evidence that the SEB_s at least at times was very delicate just *p* the Hollow and comparatively strong just *f* it, somewhat as in 1939.[2]

Strong Disturbance in *STrZ*

An unexpected feature of the apparition was a very conspicuous area in the *STrZ*. Plates XV (below) and XVI indicate its history.

Early data are few. The area was first seen by Reese on October 5, though it *may* already have been several weeks old. It resembled the Red Spot in shape and was far darker if less long. On October 17 Reese did not see any Disturbance, but on November 3 it was much as a month before. During November it expanded and became longer than the Red Spot.

On December 1 Vaughn recorded a detail which had probably developed since November 19, namely a bright space between the Disturbance and the *STB*. Haas confirmed this space on December 4, and it continued present during the balance of the apparition except that the *p* and *f* ends of the Disturbance were often joined to the *STB*. On December 1 and 4 a very dark mark connected the middle portion of the Disturbance to the *STB*. This mark was not surely present on December 9, and on December 16 it was depicted to be no longer in contact with the belt. It will be noted (Plate XVI) that the 18-inch refractor showed much structure in the Disturbance to Haas. In mid-January the area was about the same as in December.

The appearance during most of February is shown by Figure 2 on Plate XVI. By February 2 the Disturbance was less conspicuous than in December and January. Several changes since December will be noted on the sketches. From February 2 to 13 the feature was at most only faintly connected to the *STB* at its *f* end, but on February 19 and during the balance of the apparition it here merged strongly into the *STB* (see drawings). The Disturbance now lay farther south in its *f* half than in its *p* half, a difference more easily seen in April.

A comparison of Figures 2 and 3 on Plate XVI will show several changes which occurred between February 24 (perhaps even 26) and March 10. Perhaps the most important change was that by March 10 the *STB* was deflected southward between the *p* and *f* ends of the Disturbance. The deflected portion was faint, except for a large spot in its *p* part. On March 27 and April 1 the appearance was similar to March 10, but the Disturbance was now darker in its *f* part than in its *p* part (extremely dark at its *f* edge on April 1 and 5). Other probable changes are a disappearance of the darker spot in the deflected that of the *STB* and a whitening of the entire space between the *STB* and the Disturbance.

Observations on April 5 and 15 revealed a close resemblance to April 1. However, the drawing of April 5 (Figure 4 on Plate XVI) shows the *f* half of the deflected portion of the *STB* missing. By April 23 the rest of the deflected portion had vanished, and a large white area about the same brightness as the *STrZ* filled the space between the *SSTB* and the Disturbance. On April 23 and 25 the

Disturbance was far darker in its *f* half than in its *p* half, but on April 30 the two parts were comparable. On April 30 Haas *suspected* that the *f* half was shifting southward toward the latitude of the *STB f* the Disturbance. Poor views subsequent to that date revealed no further changes, the last observation falling on May 24.

The behaviour of the belts near the Disturbance was curious and perhaps very significant for the interpretation of the nature of the visible surface of Jupiter. From at latest November 18—perhaps many weeks sooner—to the end of the apparition the SEB_s was deflected southward into the *p* end of the Disturbance and was redeflected northward to its normal latitude at the *f* end (Plate XVI). The latter statement was not really true in November and December, for then the Red Spot Hollow closely followed the Disturbance and forced the SEB_s northward to a juncture with the SEB_n (Figure 1 on Plate XVI). The SEB_s *p* the Disturbance showed a sharp bend in December-February, a smooth curve in March-April (Plate XVI). The belt grew very faint with increasing longitude *p* the Disturbance; the portion *f* the Disturbance showed increasing strength with increasing distance from the *p* end of the Red Spot Hollow. The intensity of the *STB* was unaffected by proximity to the Disturbance as late as February 2, but on February 13 it was very dark *f* that feature. This aspect was gone on February 19 but present again on February 24. From March 10 to April 23 the belt was very dark both *p* and *f* the Disturbance (and faint or absent in between). These intense sections of the *STB* now lightened, being only as dark as the Disturbance on April 30 and lighter than it on May 14. From March 10 to April 5 (or perhaps 15) the *SSTB* was deflected northward into the deflected portion of the *STB* (Figures 3 and 4 on Plate XVII); the *SSTB* resumed at its normal latitude south of the *f* part of the Disturbance. On April 23 and later this belt was here undeflected.

The question of whether this Disturbance is the historical South Tropical Disturbance has appeared unanswerable to Peek.[4] It is interesting that the *average* motion of the suspected South Tropical Disturbance followed by Johnson and Haas in July-September, 1940,[3] would have brought it to *near* the proper longitude by October, 1941, for such an identity of the two objects.

Rotation Periods

These were derived in the usual way from visual *CM* transits[1,2]. Our group observed about 520 transits, not enough to give complete coverage of the planet; and the difficulties remarked on by Johnson[3] were serious. Drifts were deduced only for marks observed at least three times and followed for at least 30 days. In the tables which follow the first column gives an identifying number to the feature. The second column describes it as dark *D* or white *W* and states whether the *p* or *f* end was watched if the centre was not. The third column gives the dates of the first and last transits. The fourth column states how many transits were observed and gives the surname initial of the observer if one observer recorded all of them. The fifth column gives the oppositional longitude, placed in parentheses if the mark was not being observed on December 8. The sixth column states the average change of longitude in 30 days, to the nearest whole degree only except for a few well-followed objects. The seventh column gives the period.

S edge *NPR*, *NNTB*, and *NTB*. A small number of transits relate to this region, but no drifts can be deduced. The dark marks existent were delicate objects with ordinary telescopes or seeing.

S edge *NTB*. In October Johnson and Vaughn obtained four transits of projections or enlargements, although Johnson had previously looked vainly in good seeing for projections. Presumably there was a slight remnant of the 1939-41 activity at this latitude.[7]

NTrZ and *N* edge *NEB*

No.	*Description*	*Terminal Dates*	*No. Transits*	$\lambda^{II}\odot$	$\Delta\lambda/30^{d}$	*Period*
1*	Wp.	Feb. 2-April 22	9H	(131°)	− 14°	$9^{h}55^{m}21^{s}$
2*	W f.	Feb. 2-April 22	9H	(139)	− 15	20
3	W	March 13-April 28	3H	(217)	− 16	19
4	D f.	March 8-April 13	5H	(217)	− 10	27
5**	W	March 15-May 10	4H	(225)	− 10	27
				Average		$9^{h}55^{m}23^{s}$

*This feature Haas saw in December and January also.

**This object grew larger while being observed.

During the first half of the apparition there were observed dark humps or projections on the edge of the belt and bright spots and dark matter in the zone. In February-April there was a series of alternating dark condensations and bright bays along the north

edge of the *NEB*, of which a few were followed well enough to give the drifts tabulated.

S EDGE *NEB* AND *N* PART *EZ*

No.	*Description*	*Terminal Dates*	*No. Transits*	λ^1 ♌	$\Delta\lambda/30^d$	*Period*
1	D	March 11-May 11	7H	(12°)	0°	$9^h50^m30^s$
2	D	Dec. 4-Feb. 2	3H	(24)	− 1	29
3	W	Sept. 14-Nov. 20	4	(31)	+ 18	54
4	D	Feb. 16-April 17	5H	(76)	− 8	19
5*	D	Dec. 22-Feb. 19	4	(168)	− 10	17
6	D	Oct. 5-Nov. 28	5	(207)	− 1	29
7	W	July 23-Sept. 21	3	(242)	+ 1	31
8	D	April 2-May 11	5H	(249)	− 2	27
9	W	June 26-Aug. 6	3	(258)	0	30
10**	W	Feb. 22-April 22	5vH	(267)	− 4	25
11	D	Aug. 27-Oct. 2	3	(285)	− 4	25
12†	D	March 19-May 11	6H	(301)	− 8	19
				Average.....		$9^h50^m28^s$
				Average without No. 3.		9 50 26

*Reese observed this mark to develop after December 18. On December 22 he found it conspicuous.

**Haas saw this feature on May 11 also.

†The appearance of this mark varied.

This current was again the most active one on the planet, and numerous alternating dark projections and bright areas of greatly varying size and intensity existed along the south edge of the *NEB*. Many of the bright areas appeared diffusely outlined, and many of them depressed the edge of the belt so as to form bays. However, even this portion of Jupiter was at times comparatively void of detail: Johnson on September 1 in good seeing commented on a lack of strong *EZ* features with $CM_1 56°$, and Haas with good views on August 24 near $CM_1 220°$ and on March 10 with $CM_1 264°$ to 307° remarked on the lack of easy detail.

The numerical values given below for four marks in this current are very unreliable, but it appears impossible to doubt that they had very large drifts in longitude relative to System I between the terminal dates. Perhaps such swiftly-moving marks are necessarily short-lived because their drifts will quickly bring them close to normal moving features. Observers of Jupiter should try to check on possible very rapid drifts by observing the same portion of the planet on closely adjacent dates.

No.	*Description*	*Terminal Dates*	*No. Transits*	$\lambda^1\odot$	$\Delta\lambda/30^d$	*Periods*
1	D	Oct. 20-Oct. 26	4	(32°)	− 80°	$9^h48^m42^s$
2	W	Oct. 20-Oct. 26	4	(52)	− 75	48 49
3	D	April 27-May 4	3H	(72)	+ 57	51 47
4	W	Oct. 24-Oct. 26	3	(282)	− 150	47 8

S part *EZ* and *SEB*. Very few transits relate to these latitudes, and no drifts are deducible.

STrZ DISTURBANCE

Description	*Terminal Dates*	*No Transits*	$\lambda^3\odot$	$\Delta\lambda/30^o$	*Period*
pS edge*	Dec. 16-May 19	13H	(106°)	− 10.°1	9^h55^m27
pN edge*	Nov. 16-May 19	16	113	− 10.2	27
centre	Nov. 19-Jan. 14	3	130	− 9	28
fN edge**	Nov. 18-May 11	18	140	− 10.0† − 20	27 13
fS edge	Feb. 19-May 23	14H	(141)	− 9† − 22	28 10

Average Nov. 16-April 7.... $9^h55^m27^s$
Average April 7-May 23....9 55 19

*Perhaps the drift was not linear.

**The *f* end may have been accelerated by an *STrZ* bright area near $\lambda_2 146°$ in December.

†It is clear that the *f* end was accelerated on or near April 7. There obviously resulted a rather rapid shortening of the Disturbance.

Though no transits were observed before November 16, Reese's drawings *suggest* that the *f* end had a near-constant longitude throughout October and November and that the *p* end moved in decreasing longitude after the Disturbance began to expand.

RED SPOT

Description	*Terminal Dates*	*No. Transits*	$\lambda^3\odot$	$\Delta\lambda/30^d$	*Period*
p end	Aug. 27-May 10	8	158°	+ 0.°9	$9^h55^m42^s$
centre	June 28-Jan. 14	20	173	− 0.5	40
f end	Aug. 3-May 10	7	187	− 0.5	λ 40

Average.... $9^h55^m41^s$

The 1937-41 drift in increasing longitude[2,3] had finally been reversed, the change having occurred near the 1941 conjunction. It is probable that the *p* end of the Spot usually drifted like the *f* end but was decelerated near March 1.

STrZ AND *N* EDGE *STB*

No.	*Description*	*Terminal Dates*	*No. Transits*	$\lambda^3\odot$	$\Delta\lambda/30^d$	*Period*
1	Df.	Jan. 19-March 16	5H	(201°)	− 4°	$9^h55^m35^s$

A few bright areas seen in the zone supplied no drifts. On October 26 Vaughn and Johnson placed the *f* end of dark matter in the *STrZ* near $\lambda_2 226°$, this feature being less conscpicuous on November 19. On November 20 Johnson placed the *p* end of some very faint dark matter in the zone at $\lambda_2 313°$. Do these observations relate to the historical South Tropical Disturbance? The implied length is about 273 degrees. There were observed a number of dark objects on or near the north edge of the *STB;* especially conspicuous ones lay near $\lambda_2 202°$ on August 3 and $\lambda_2 286°$ on October 26. Dark matter just *f* the Red Spot Hollow, or even penetrating it, may have been intermittently present during much of the apparition.

STB AND *STeZ*

No.	*Description*	*Terminal Dates*	*No. Transits*	$\lambda^3\delta^\rho$	$\Delta\lambda/30^d$	*Period*
1*	D	Nov. 19-Jan. 19	3	183°	− 29°	9^h55^m 1^s
2	Dp.	March 16-April 28	4H	(282)	− 30	0^s
					Average....	9^h55^m 0^s

*This dark projection on the south edge of the *STB* was drawn forming as a hump by Reese on November 3 and was shown fully developed on November 18.

There was much dark matter in the *STeZ*, including projections and columns based upon the south edge of *STB*.

SSTB. Johnson recorded a few dark spots at the north edge of this belt, but drifts are unobtainable.

"Personal Equation" in Transits

Peek has commented on systematic differences in times among different transit-observers.[8] A program of satellite-and-shadow-transits discussed by Johnson[9] can reveal such systematic differences but only slowly.

The fact that between October 15 and November 20 Vaughn and Johnson 25 times *independently* observed the transit of the same mark on the same date with the same telescope (either Smith's five-inch or Vaughn's seven-inch) permitted an alternate approach to the problem. The average value of the difference in their times in the sense Johnson-Vaughn and its probable error are $3^m.0 \pm 0.4^m$, in Jovian longitude $1°.8 \pm 0°.2$. It appears impossible to doubt that Johnson recorded transits significantly later than Vaughn. No

dependency of the value of their difference on the character of the features is apparent.

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110 N. Madison St.,
Upper Darby, Pa.
June 25, 1945.

A POSSIBLE ORIGIN OF COMETS

By H. Boyd Brydon

AS YET no adequate theory of the origin of comets has been devised. Jeans, in *The Universe Around Us,* 4th ed., p. 245, considers them to be composed of débris from the formation of the planets. Olivier, in *Comets,* of which a new edition would be very welcome, attributes them to matter ejected from the polar regions of the sun during the period of the hypothetical dynamic encounter of the sun and a passing star.

The single star theory of the solar system recently suggested by von Weizsäcker, as summarized by Gamow and Hynek (*Astroph. Jour.,* vol. 101, March 1945, p. 249) does not invalidate these hypotheses. Admittedly, however, the brevity of a comet's life prevents the full acceptance of either. No comet is known that gives evidence indicating an origin so ancient. On the contrary, the discovery of new comets—about 300 per century (Russell, Dugan and Stewart: *Astronomy,* p. 424)—strongly suggests that the evolution of comets is a continuing process.

Now, as Dr. Olivier cogently remarks, the matter forming these new comets must have come from somewhere. Is it not possible that its source may be found in eruptive solar prominences?

It is true that the remarkable McMath-Hulbert cinématographs of these prominences appear to show that some of the erupted matter returns to the chromosphere, but there is no evidence that all of it does so—some escapes into space.

Discussing the probable masses of comets Russell, Dugan and Stewart say, p. 430, "all that can be said with certainty at present is that it appears probable that the mass of even a large comet is less than a millionth part of the earth's mass"; that is, is less than 6×10^{9} tons, or 5.4×10^{12} kg. Later, p. 444, on certain assumptions, they compute a mass of 2.5×10^{7} tons, or 2.3×10^{10} kg. for Halley's comet. How do these masses compare with the mass of an eruptive prominence?

Dr. O. Mohler of McMath-Hulbert Observatory kindly informs me by letter that investigations by Pannekoek and Doorn, quoted

by Unsöld in ***Physik der Sternatmosphären,*** indicate that the mass of an average eruptive prominence is about 3.5×10^{15} kg. Dr. Mohler says, "If we accept this result, the mass of the prominence of 1919, May 29 (the largest so far observed) is approximately four times greater," or 1.4×10^{16} kg.

From these figures the mass of an average prominence appears to be about 650 times, and of a large prominence 2,600 times greater than the probable maximum mass of "even a large comet".

Further: Although, as might be expected, cometary spectra indicate the presence of carbon-hydrogen and carbon-nitrogen and other compounds (see *A Synopsis of Cometary Spectra,* Frank S. Hogg, this JOURNAL, vol. XXIII, p. 55, 1929,) they contain no element not found in the spectra of eruptive prominences.

While the suggestion here outlined may prove to be unacceptable on other grounds, on the score of mass and composition it would seem possible that comets may have their origin in eruptive solar prominences. In that case the time difficulty, so formidable an objection to both the theories mentioned, would cease to exist.

2390 Oak Bay Ave.,
Victoria, B.C.

HER MAJESTY'S MAGNETICAL AND METEOROLOGICAL OBSERVATORY

By A. D. THIESSEN

(*Continued from page 369, this volume*)

The northern lights, the northern magnetic survey, and the photographic records are mentioned again in the next letter.

At this time it was known that the British Government would soon withdraw its support of the Toronto Observatory, and that plans would have to be made for taking over the cost of administration by the Province of Canada West. The government of the Honourable Robert Baldwin was at this time under strong attack from a group deriving its strength from the western agrarian sections of the Province. This group, which later came to be known as the "Clear Grits", thought that Baldwin's government was spending too much money and it was difficult for him to incur the added cost of the Magnetic Observatory. However, Baldwin's successor, Sir Francis Hincks, took over the Observatory, and not only paid for the instruments, but "purchased at fair prices a number of my [Lefroy's] books, and compensated [him] liberally for the addition [he] had made to the cottage."[93] It is the "Clear Grits" Lefroy has in mind when he speaks of "a very illiberal ignorant faction" that is "getting the upper hand here and using retrenchment as a party cry."

Toronto, May 16, 1850.

My dear Colonel,

I forward herewith nearly the whole MS of the Athabasca observations, the meteorological and one magnetical section being the only ones not included. It is written on one side only, and will not form altogether, I should think, more than fifty or sixty pages of print, exclusive of the abstracts, which with the Term days and Disturbances, I reckon at 100 or 150 pages, according to the way in which the latter are printed (single or double column). I shall be very glad if you can find leisure to read the section of 'General remarks and recapitulation', referring as you go along to plates V and VI, as it will render it unnecessary for me to occupy your time now, with the conclusions to which I think these obser-

[93]Lefroy, *Autobiography*, p. 123.

vations lead, and in which I am so much interested. That it was perfectly proper, and within my province, to enter at length, and once for all, into the comparison of these northern observations with those of Toronto, will not, I suppose, be questioned; but in bringing together the scattered conclusions, I have felt doubtful whether they do not come under the class of deductions which you prefer communicating to the Royal Society, to developing in the special publications of your office, and whether you will not prefer in their case that course. Three years ago you suggested its adoption, if any striking circumstances came to light, but in the first place it was difficult to form a judgment on the claim of the observations to the honor of especial attention until their reduction and comparison was quite completed. In the second, I had no wish for any other vehicle of publication than our own regular one, to which we must look with greater interest than to any other. However, it does not appear to me that they bring out peculiarities sufficiently novel and striking to be worth communication, and that I have been led on into conclusions which are so far speculative that you may think them inappropriate in a mere report while I am desirous not to forego them. Will you oblige me then by expressing your views on the matter. If thrown into the form of a communication to the Royal Society, I should wish to omit, for the mode of publication originally proposed, the whole of the details of adjustments and other introductory matter, but to leave the rest just as it is. I should in such case however go further into the subject of the mean irregular movements at Toronto than was necessary for comparison, and in fact use up the whole materials I have. There will be plenty of time before the winter, to return the papers to me for necessary alterations, if this is the course you recommend. It is rather curious that Franklin and Richardson in their northern observations, missed the detection of the magnetical peculiarities of the night (at least I believe they did). They observed from 6 a.m. to midnight only. That we were more fortunate is due to the training of these observatories, which has made observation throughout the 24 hours the normal and natural course. Lake Athabasca is 1720 miles from Toronto. Fort Simpson 1830 miles, in the direct line. They are 410 miles apart, which is more than half of the distance of Fort Simpson from Sitka. Their distance from Toronto, and difference of latitude, would prepare me for great differences in the magnitude of the regular and irregular changes, but hardly I think for such differences in their law, but the distance from Sitka would hardly prepare me for so great difference as those in these reports, even when referred to that station. This the mean regular diurnal change of Declination at Sitka in April 1844 was 9.8′, in May 13.2′. It was 38′ in April and 33′ in May at Fort Simpson. It was 33′ upon the union of all the observations. It shews how little mere geographical position has to do with these things. There is another striking point of contrast between them. We had aurora on 73 nights out of 162 at the two stations. I can only find three or four notices of it at Sitka in all that time.

I am happy to say that Sir George Simpson enters with his usual good nature into my plans about the Aurora, and will recommend an attention to my request for a register, at every post he visits. There has been much less of it this spring than there was in 1848 and 1849. Our people at the different commands are

mostly very zealous and ready to assist. This reminds me to say that I forgot to address one of those Reports to Sir John Richardson. Will you oblige me by having one sent to him, my stock is exhausted?

I mentioned in my last letter that I have had a reply from Mr Baldwin. He says that he has talked the matter over with his colleagues, and that although there is a great outcry for retrenchment, and the Government is ill able to incur fresh expenses (it is known to be much embarrassed) yet rather than permit observations of so valuable a nature to fall to the ground, they will be prepared to advise His Excellency to recommend to the provincial parliament an appropriation not exceeding £300 Cy a year (the sum I named). He would however greatly prefer to let the observations be carried by the University upon its own Funds. He concluded by saying "this is of course non-official. When a formal official communication to the Government is to be made it will be through the Provincial Secretary, and it had better embrace the considerations as to Instruments, etc., about which we conversed." This alludes to the expectation I held out that the Government in giving over the Establishment will include all the instruments required for the observations desired for the future. It by no means follows that even a recommendation from the Government would secure the grant. A very illiberal ignorant faction is getting the upper hand here and using retrenchment as a party cry. It has been with great difficulty that funds for the completion of the provincial Geological Survey have been obtained by Mr. Logan. I think our best hope lies in the success of an application to be made by the Americans, and will endeavour to bring it about as soon as we are fairly started in the Photographic experiment. The *Pearl* and *Lady Elgin* are late in making their appearance. A good many other vessels are in.

I have been obliged to get nearly the whole of the floor of the observatory taken up, in order to lay down new joists. The old ones were completely destroyed by dry rot. Some of them could be broken in two with ease. As it can never be done so conveniently as during preparations which necessitated, at all events raising nearly one third of the floor, I thought it right to take advantage of the opportunity. I am happy to say that the new arrangements will allow the retention of the Balance VF and Induction Inclinometer which seems important, for comparison with the self-registering instruments.

Your Transit Instrument had a narrow escape the other day. I had sent it to an instrument maker for a new set of spider lines. Within twelve hours after its return, his house and a dozen or two of others, were burnt to the ground. We had a great fire, one might say, our annual great fire.

I was happy to hear good accounts from Smythe of the improvement of your health, which I trust is permanent.

Believe me, very faithfully yours,
J. H. Lefroy.

Will you correct with your pen the distance from Fort Simpson to Lake Athabasca called 365 miles? It should be 410 miles. General remarks at the end.

The next three letters deal with the arrival of further instruments from England.

Magnetic Observatory,
Toronto, May 22nd, 1850

Serjt James Walker, 9th Battalion, Royal Artillery, is directed to proceed from hence to Montreal, reporting himself on arrival to the Brigade Major of the Royal Artillery, and also reporting himself both going and returning to the Commanding Officer at Kingston, if the arrangements of the boat permit it. He is to deliver the letter with which he is provided to Messrs MacPherson Crane and Co. and take steps to land and enter at the Custom House, two cases of Magnetic Instruments, on board the *Pearl*, and three cases ditto on board the *Lady Elgin*, making application to Major Sandham for assistance if necessary. He is to pass these cases through the Custom House if possible, if there is any difficulty in this to bring them on in bond. It will be his particular duty to take care of these five cases and see that they receive no injury in the forwarding to Toronto as they contain Instruments of great value, and of a delicate nature.

He will report to Major Sandham the completion of the arrangements and make application for a route to return to Toronto without loss of time, including military transport for the cases, if not in bond.

J. H. Lefroy,
Captn. R. A.

Magnetic Observatory,
Toronto, May 22nd, 1850.

Gentlemen,

Having heard of the arrival of the *Pearl* and *Lady Elgin* at Quebec, I have thought it advisable to send one of my assistants, Serjt. J. Walker, to Montreal with instructions to expedite if possible the discharge of the cases of Instruments on board of those vessels, and to accompany them to Toronto for their better security. I shall feel obliged by your co-operation in my object of securing their safe and speedy transmission. It would probably save time and trouble, to pass them through the Custom House at Montreal, for which I presume the Invoices in your possession will be sufficient, and he is instructed to do so if possible. In this case it will not be necessary to trouble you to forward them as he will get military transport. If there is any difficulty about this, I must request you to proceed as you would have done without his presence, but to give him instructions how he is to proceed, and how he is to get the goods started as soon as possible, and to keep them in view in their journey to Toronto.

I remain, Gentlemen,
Your most obedient Servant,
J. H. Lefroy.

If the goods on board the *Pearl* are under any specified conditions, you will be good enough to take steps not to forfeit the right to recover damages under

insurance if any such right exists at present. The instruments are insured, and there is much reason to fear that they will be in a damaged condition.

J. H. L.

Messrs MacPherson Crane and Co.,
Montreal.

Toronto,
23 May, 1850

My dear Colonel,

The *Pearl* and the *Lady Elgin* reached Quebec on the 16th and 18th inst., respectively. I have sent Serjeant Walker to Montreal for the Instruments about which I have heard from Mr. Ronalds, and thought so much, due to his anxiety. I was besides very glad to give the man a holiday which he needed. We shall be ready for them, but as the room will not be thoroughly dry for three weeks after plastering, it will not be advisable to begin the chemical processes at once, even if otherwise able, it will probably take that time to start.

The inclosed short paper was written at the request of Dr. Silliman for the *American Journal*. There is nothing of interest in it, except a table of comparative frequency of disturbances of given amounts, which you will I hope find useful, and I may perhaps add, the account of two material deviations from Mr. Brooke's arrangements, which I regard as improvements, namely, the doing away with the external cylinder, and the making the cylinder revolve but once in 24 hours instead of twice. I send one to Mr. Brooke, from whom I have not heard for an age, and have not the least idea when his new instrument may be expected.

I beg to inclose a receipt of Serjt. Johnston for £3 12, which I have paid him for work done. He has another account, not yet paid, for £3 18. I am also desirous to pay Serjt. Walker £4 10 for his extra work since he has been the senior N.C. Officer. He does not get the addition to his pay which Serjt. Johnston got during the last two or three years, although he does the same duties, and most fairly earns a gratuity for extra work. The senior N.C. Officer has a great deal more to do than any of the others, owing to the multiplicity of formal returns required, which are all done after office hours. These three payments amount to £12 and I shall be much obliged if you will tell me whether I may go any further. Johnston works at a lower rate than any one less thoroughly acquainted with the details could do, and I should like much to employ him to complete the discussion of our mean irregular fluctuations, as well as some other discussions, but I am unwilling without better knowledge to trespass too far on the funds you can apply to these purposes.

Believe me, Very faithfully yours,
J. H. Lefroy.

The next few letters tell their own story.

Toronto, June 6, 1850

My dear Colonel,

I am much obliged to you for your letter of the 15th (which I am sorry to see was written by another hand) and have no hesitation in admitting that since

the letter to which you refer I have altered my mind in some measure as regards the duty of returning to the Regiment, while reasons for desiring to remain here have multiplied, personal reasons and scientific ones. No one would, I think, willingly bring into operation a system so full of promise and of interest as the new photographic methods of magnetical registration, to let another reap the fruits of it. If creditably carried out, it will secure all the distinction to this Observatory which its best friends can wish, and it is above all the instrument of the particular enquiry which circumstances seem to lead me into, and which I wish to follow up: The Aurora and its relations. I am fully aware that the prizes of such undertakings as ours are only to be reached by persevering in them to their natural termination, and neither as regards the Photographic series or the Absolute determinations wish to introduce another hand if it can be helped. I am besides personally strongly interested in both, and in the progress of magnetism in every way.

I am much too happy among my friends and connections here to wish to quit Toronto, as a place of residence.

I would only desire that if private circumstances require me to visit England, as I think they will do, my reengagement may be no impediment, provided I can arrange for the conduct of the work, and that I may be at liberty to retire altogether, with due regard to your convenience, should I so desire, before the expiration of the period.

I read the Paper on the Absolute Determinations with great interest and pleasure. I presume you stopped short at April 1849 not to introduce a new question by the apparent increase of H. Force in May and June. The continuation of the series down to May 1850, gives confirmation of the annual change and does not in any way weaken the inference as to a secular one. I inclose a memorandum of the results. There appears to me to be an error in the value you have taken for April 1849. The observations give 3.5272 instead of 3.5260 and that is the value in all our books. There are some inaccuracies in the details of the paper.

The no. of vibrations was 400, not 300, universally; 300 would not be enough.

The initial semi-arc was always nearly, but not absolutely the same. It was usually more than 25′ (arc 50′) 25′ would be nearer the average. I don't mean that this is of any consequence, but I like to see details precise. I think it would have been well to state that each mean depends upon 9 or 10 independent determinations of m/x and 6 or 8 of mx, and that the magnet was vibrated before and after the deflections of each day. These points are I think important in the question of the value of the results.

There is misstatement as to the Dip which was never taken as you describe. It was always observed a.m. and p.m. of the same days, not a.m. on one day and p.m. another. Consequently instead of each result being the mean of 8 or 9 determinations it is the mean of 16 or 18. As I read the account at page 287, it appears to me to be calculated, I have no doubt unintentionally, of course, to convey the impression that my business was the mere mechanical one of making the observations: that they were entirely reduced and calculated at Woolwich. In point of fact not only was this labour performed here but the system and

method from first to last (except as far as defined by printed forms) was left for us to settle for ourselves. I was responsible for the adoption of the diurnal mean instead of the monthly one, and had it not been the best standard of reduction, should no doubt have had the trouble of reducing the observations to some other. I think therefore that a few words might have been introduced there without injury to the paper, to the effect that the manner of conducting the observations having been left to the discretion of the officer in charge of the Observatory, he had adopted such and such a system, and that the results were to be the subject of the paper. The responsibility devolving on the officer may perhaps be implied, but I do not see why it should not be expressed. I am much obliged to you for the manner in which at p. 299 you characterize the observations, and if I have alluded to the mode of expression adopted in the other instance it is not in the spirit of complaint, but simply to give expression feeling which whether well or ill founded, I think I need not refrain from expressing. I am not particularly tenacious, or disposed to take things in a wrong spirit, but I should like to see a way devised of reconciling the supervision and the responsibility of your office with some definite opening for the junior officers, such as the duty of accounting for example, for all the details not only to you but to the world, would be, and that of drawing at least the more general and elementary conclusions, if not of being exclusively entrusted with certain subdivisions of the subjects investigated.

I must say that the extent and theoretical importance of the conclusions derived from these observations, much exceed what I anticipated, and that I was much struck with the ingenuity with which they were interrogated. I shall most glad[ly] continue the series as long as I am permitted, and feel more assured than ever that the labour bestowed upon them will be amply repaid. I have written at considerable length to Prof. Henry and mentioned the results. I have also written to Bache, but to no one else as yet. I propose to write to Loomis, Pierce, Bond, and Lovering.

Believe me, Very faithfully yours

J. H. LEFROY

Smithsonian Institution,
June 8, 1850

Capt. J. H. Lefroy,
Dear Sir,

Your communication of the 3d instant has just been received and the same mail has brought Ronalds' Report on the Observatory at Kew, with a copy of Col. Sabine's paper on the secular changes of the Magnetic Forces. I also received about ten days since a letter from Col. Sabine, suggesting the propriety on the part of the Smithsonian Institution of an expression of an opinion relative to the importance of continuing the observations at Toronto. The Board of Regents of the Smithsonian Institution met last Saturday, and I placed before them the volumes of your observations and it was resolved that thanks be returned to Her Majesty's Government for the gift, with the expression of a hope that the valuable establishment at Toronto might be continued. This letter will be signed by the Chancellor and the Secretary of the Smithsonian Institution, the former

being also Vice-President { since President / [Initialled I.H.S.] } of the United States. I will also have a copy of your letter sent to the American Philosophical Society, the American Academy, and also place it before the American Association at the meeting to be held at New Haven in August next.

I am much pleased with your exertion in regard to extending our knowledge of the phenomena of the Aurora Borealis. We shall be happy to coöperate in any way in our power, in extending the system over the United States. We have quite a number of Observations, which we will place at your disposal if you desire it, to be incorporated in your returns. It might perhaps stimulate the observers, could a list of their names be given in your communication, and copies for distribution sent to us. Or if you could make a report to us, it would be printed at the expense of the Smithsonian Institution. I merely throw this out as a suggestion, my only object being to advance the cause of science.

I shall return the Photographic registers, and your paper on the Aurora, at the beginning of the week.

I regret to inform you that I have been unable to do anything with regard to the establishment of magnetic observations in this institution. An accident happened to the building several months ago, which will retard, perhaps for some years the completion of it, and add nearly $50,000 to the cost. We have not therefore, at present, a place in which to set up the instruments. I will however endeavour to interest the officers at Georgetown College in the enterprize, and if I do not succeed in this, we will procure the instruments as soon as our funds will admit and make such a temporary disposition of them, as will allow results to be obtained.

I remain, etc.,

JOSEPH HENRY
Secretary, Smithsonian Inst.

With regard to this proposition, I am about to accept it, subject to approval by Colonel Sabine. The Observations being almost exclusively American, it seems appropriate to publish them first in America, and I desire to make any use that may appear advantageous of the Toronto records. JHL.

Smithsonian Institution,
June 14, 1850.

Capt J. H. Lefroy,
Dear Sir,

I return your paper on the Aurora, after having kept it a very long time. [The paper referred to, was a section of the Athabasca observations, embracing meteorology generally, and Aurora. 18 or 19 pages.] I shall endeavour as soon as our funds will permit, to establish a self-registering apparatus in connexion with the Smithsonian or some other Institution. I applied to Georgetown College situated two miles from this, but they inform me that the condition of the college is such as to render it impossible for them to engage in the observations. I have two other resources to which I shall apply. The Smithsonian building, I now

think, will be in a condition to receive the apparatus about the end of the present year, and if we cannot induce others to undertake the charge of the observations before that time, we shall then commence them.

I have read with much attention your paper on the Aurora, and think it one of the most interesting additions to our knowledge of that subject I have ever met with. There are one or two remarks that might perhaps be modified. Speaking of the effects of the Aurora on the weather, you observe in substance "that if it is entirely above the atmosphere, there would be no reason to expect any connexion between it and the weather." If the phenomenon be caused by a disturbance of the electrical equilibrium, then by the inductive action at a distance, it might affect the meteor even though the meteor be many miles beyond the confines of our atmosphere. Again, I think you are a little too strong in your remark that the magnetic Disturbances and the Aurora are not related as cause and effect. If the Aurora consists of discharges of electricity from the upper regions downwards to the earth, in some cases an equilibrium may be produced by the forces acting equally on either side of the meridian, or nearly so. This appears to me to be a point not yet settled.

I remain, etc.,

JOSEPH HENRY,
Secretary, Smithsonian Institution

Observatory, Cambridge,
July 23, 1850.

From Prof. W. C. Bond.

My dear Sir,

Your favour of 10 June I duly received. I have delayed answering it in the hope of being enabled to give you a definite answer in regard to the time when we should be prepared to recommence magnetic observations. Although our prospects are very encouraging for the coming year, we cannot at this moment state the exact time.

I was much pleased with the specimens of your Photographic registers. With us limited as we are, and always have been, in respect to assistants, the self-registering apparatus is indispensable to a long continued series of magnetic observations. I have never lost sight of the subject since our coöperation with Captn. Riddell, but the continuance of these observations with a single assistant I found to be altogether incompatible with other duties if pursued according to the old method.

We are now assured of ample provision for Instruments, and in all probability shall be enabled to put up our additional buildings, and commence magnetic observations next year.

I have been exerting myself to make our friends sensible of the importance of prosecuting a series of meteorological and magnetical observations systematically. The loss of Toronto Observatory would, I fear, prove fatal to the development of an extended system of observation which is now on the eve of being carried into effect throughout the United States. We have Dr. Bache,

Superintendent of the Coast Survey, Prof. Henry of the Smithsonian Institution, Col. Abort (?) of the topographical Engineers, and nearly all the officers of the army and navy ready to coöperate on the part of the general government. New York and Massachusetts State governments are favorably disposed toward the meteorological portion of the design, and will probably soon see the utility of combining magnetical observations with the course already adopted. The American Academy has recently decided to resume regular observations with their own apparatus. Toronto cannot in my opinion abandon the field without causing irreparable mischief.

Prof. Guyot is precisely the right sort of man to take charge of the meteorological department. The Governor of this state has assigned the charge of arranging the stations in this state to Hon. Wm. Mitchell of Nantucket who will no doubt recommend Prof. Guyot for the situation of General Superintendent of the 12 stations to be established in Massachusetts. The greater part of these stations will be fixed at Colleges, Academies, and Normal schools.

I have talked over the matter with Prof. Lovering; he is ready to assist to the utmost of his ability.

The plan which I have proposed for our magnetic observatory embraces a subterranean apartment of 24 × 40 feet. The situation and nature of the soil are favourable to such an arrangement. We are in connection by telegraph wires with the Seaton Station at Washington, and have a clock which is readily connected with the Electro-telegraph lines passing through New York, Philadelphia and Baltimore to Washington, so that we shall be prepared occasionally to communicate during remarkable perturbations and Auroral exhibitions. We are now constructing a machine whereby rotary motion is governed or regulated by the vibrating pendulum. Some safe regulation of the conical pendulum, fans or balance was found to be indispensable for the determination of the electric wave time. It would occupy more time than I could well spare to describe in detail to you the provision we are endeavouring to make for our magnetical operations. I trust that the brief outline given above will serve to assure you that we are interested in the subject which you have so much at heart, and desirous of coöperating with you. I should feel much obliged if you could occasionally inform me of your progress in photographic registering, as on its success must depend our prosecution of continuous magnetic and meteorological observation.

With great regard, etc.,

To Capt Lefroy — Wm C. Bond

Toronto, June 26, 1850.

(To F. Ronalds)
My dear Sir,

Although I have not yet taken the important step of preparing a plate, and executing a trace in either of your magnetographs, I am sufficiently advanced perhaps to make a report of progress acceptable next to putting the instruments correctly together. The subject that has cost me most consideration is the determination of the lineal value on the plates of a given change of Force or v, v,

the change of Force measured by a given lineal movement. As regards the *V* Force instrument I cannot gather from your letter any intimation of your views on the matter. You say in general terms that the supports of the magnet are similar to those of Lloyd's Instrument but I find no microscopes or micrometers for measuring the angular movement of the bar, and even if these existed the unknown coefficient of magnifying power would leave the movements of the plate undetermined. I need not tell you that in Lloyd's original mode of adjustment the quantities involved in the coefficient of the instrument are (1) The dip. (2) The angular value of the space traversed by the moveable wire in one revolution of the micrometer head. (3) The ratio T'^2/T^2 of the times of vibration of the magnet in the vertical and horizontal planes. Whether the latter can be determined correctly unless the magnet carries the same appendages in both positions, and whether even then the circumstance of the weight and arm being horizontal in one and vertical in the other will not make a difference is a point I cannot venture to determine; it involves mechanical considerations beyond me, but I have made the observations. (1) In the vertical plane without *T* appendages—21.74 *S*. (2) In horizontal plane with appendages 22.01 *S*. (3) In vertical plane with appendages—5.02 *S*. The last is too small, and the counterpoise must be raised, but admitting the sufficiency of this part of the arrangement, I cannot perceive how the absence of any provision for measuring the angular movements is to be supplied. The whole accuracy of the coefficient depends upon this. If I could venture to take such a liberty I should adopt Dr. Bache's modification, attach a small plane mirror in the prolongation of the exterior axis and make use of a detached telescope with a scale. This will require a glazed aperture in the magnet box near the upper edges, and if the knife edges are in very hard steel, as I suppose is the case, will be attended with difficulty; in particular, with the risk of altering their adjustment. I am here thinking of drilling a hole in one; perhaps we can fix a socket to the end of it, in which case there cannot be much risk. On the whole the measure seems to be too important to be executed rashly, particularly as I have not received the plates of this instrument or your directions concerning it. I shall raise the counterpoise to increase its sensibility and then commence to use it. With respect to the Bifilar the first consideration was the value of F/A, the ratio of the torsion force to the force of the earth, proper to be adopted. It appears to me that if the sensibility of the Instrument or rather the scale value is to be sufficient to measure ordinary movements, this ratio must be larger than we have ever before had it. You say by way of illustrating the magnifying power of the camera, that a chord of 0.75 inch in the angular movement of the screen, gives 3 inches on the plate, that is to say nearly the whole of the effective width. The length of radius *V* I find to be 8.7 inches to which a chord of 0.75 inch measures an angle of 5° 42′ nearly; if we take our old Bifilar as criterion, such an angular movement corresponds to a change of scale reading of 740 divisions = 0.078 *X*, a quantity only exceeded 3 or 4 times in ten years. We may take the mean diurnal change or difference between the highest and lowest mean diurnal value of the year at *.0025 more in summer, less

*It is hardly as much.

in winter; upon the same scale, this quantity will give an ordinate of only .096 inch, and a single division on your ordinate scale will correspond to .00052 X by making the ratio F/G larger than it was under the adjustment in question, we can increase this a little, and I intend in consequence to have the angle V as nearly [paper torn here], as I can make it (sin $V = F/G = .94$), which will give 3 inches $= .042\ x$, and allow about 0.38 inch for the mean daily movement, leaving some few disturbances to exceed the width of the plate. (2) I consider, I confess, this too small a scale for much practical value. I do not see with Colonel Sabine that Instruments of this nature should be calculated to measure the most extreme movements and think a good large scale for ordinary movements much more important; but this may be a matter of opinion. I tried to adjust the Instrument with Captain Younghusband's mirror, but found it insufficient, the mirror must have a motion of 90° or 100° at least, and should have a free rotation; his had not a greater command than 70° or 80° owing to the arms of the stirrup. After consideration I had it altered altogether. It was as fig. (1). It is as fig. (2). [Omitted here.]

The central axis, or connection between the stirrup and the Y's is a light tube, the mirror turns freely upon it and can be set by a screw behind, it is rather more than half an ounce heavier than the old stirrup (245 grs.) which is no great matter, the weight to be suspended being about 1½ lbs. The next thing to be thought of is a reading telescope. I first tried a detached one, but it was inconvenient and in the way; I have now attached an arm to the box V, projecting about 15 in. between P^N and P^E of your plate and carrying a small telescope and scale which originally belonged to a transportable Bifilar; it will also be supported by stays permanently connected with the principal pillars, and these, I consider, be sufficiently firm to allow the instrument to be used independently of its self-registering properties. All this was not completed until this morning: my intention is to complete an adjustment of the usual kind by aid of this telescope and scale, then compile optical adjustments, then (removing temporarily the slider case E) to determine very carefully the scale value by the method of deflections, then ascertain experimentally the lineal value on the plate of a given range of the scale of the telescope. This will complete the adjustment, but I hope you will be prepared to see some little time consumed in such processes. I expect to have both instruments at work by the first week in July. There are one or two minor points I feel hesitant about: (1) It appears essential that the coating be done by daylight, how is this to be reconciled with two plates in a day. What two homonymous hours will be least inconvenient and render the continuous application of the instruments least onerous, is a point I have not quite determined, but I think I shall adopt 6 a.m. and 6 p.m., or 10 a.m. and 10 p.m. The hours 6 a.m. and 6 p.m. are hours of daylight for only 6 months of the year. Can the plates be coated by artificial light? (2) I do not see any way of making the hour on the dial plate or wheel I correspond to time for any other hour of starting than noon or thereabouts—is there any way short of shifting the points to which the chain and gut line are attached? (3) Is there any objection to making the time 24 h. instead of 12 hrs., if it can be done. I did it with Mr. Brooke's instrument, and found it a great improvement. The whole diurnal curve became continuous, and

its symmetrical character much more apparent. I don't know that in the case of your instrument a corresponding alteration is possible, but wish to submit the matter for consideration. (4) Pine olive oil costs here 12s. Cy (About 10s. Sterling) a gallon. I find that each lamp holds a quart, and supposing that the average consumption of two lamps, including waste is 1 quart a day this item would be £46 Cy a year. To this must be added the consumption of camphine for three of Mr. Brooke's lamps, (if not four) burning 24h. a day. 40 gallons at 6s. Cy = £12 Cy, an expense for light of £58 a year independently of the ordinary consumption of the office a portion of which is also paid for out of our funds for contingent expenses. Now this is more than our present funds will allow, and an addition must be demanded for the express purpose of conducting these observations, but my object in going into these details is to enquire whether there is any reason to doubt the sufficiency of gas, because I am *almost* persuaded that I can manufacture gas for our own use, at a cheaper rate, at all events mean to give the practicability of doing so, careful consideration. It would be a great comfort and convenience. Coal is cheap here: the cash price of gas in the town is 16s 8d Cy per 1000 feet, but our distance from the nearest lamp is such that I have no hope of the Company laying it on. (5) You have sent altogether 18 plates. Col. Sabine mentioned I think once that each plate is expected to stand 30 or 40 cleanings. This seems to me a very large number, but as 4 plates must be cleaned every 24 hours, the supply at 35 cleanings per plate will only last 160 days; i.e. from 1st July to 8 December; do you intend to send more? Full sized plates of 8½ × 5½ inches cost here £2 8s Sterling per dozen. I doubt if they can be procured of your dimensions, but they could doubtless be made to order in New York, probably at less than the cost of sending them out from England. (6) The iodine appears a little damp although I have not unstoppered the bottle, it adheres to the side of it. Should it not be perfectly dry? (7) Is there any mechanical advantage in having the knife edge of the *V.F.* bar excentric (*sic*) so as to require the counterpoise to be at [. . . . paper torn], ... dips. I was at first puzzled by this, taking for granted [. . . . . .] at the South end. I have now I think proposed [. . . . . .] I hope the best for my success but I am,

Believe me, yours very truly,

J. H. LEFROY

Toronto, June 27, 1850.

My dear Sir,

I must add a few words to my long letter to make the remark that I wish I had received your short account or epitome of Electro-Meteorological and other observations at Kew, a little earlier, so as to have avoided an arrangement and turn of expression in that little paper of mine, calculated to mislead those who know nothing about it, to suppose that your invention of a self-recording instrument was subsequent to Mr. Brooke's. You can afford to smile at the infinitesimal

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[A letter from Capt. Lefroy to Mr. Ronalds — Recd. at Woolwich July 18, 1850.]

loss of credit which can possibly accrue from a paper so unimportant—but since there is a right and a wrong in these matters and since I gather from that pamphlet that you have sometimes had to contend for the priority or the originality of your inventions, I regret to have unintentionally failed to do you justice in the matter, which would have been perfectly easy, had I thought of anything more than presenting a sketch of the invention, to justify the general heading.

I am very desirous of testing Barlow's observations on the natural currents of electricity circulating in the Earth, and the correspondence or analogy of their fluctuations both regular and irregular, with those of the Declination needle. Within a mile and a half of us, we have a line of telegraph wire extending 600 or 800 miles mostly in the NE direction. It would not cost more than £50 to connect the Observatory with it and if we could then, during the night and also during the 50 or 60 Sundays and holidays, register these movements photographically like the others, we should indeed be complete for our purposes of magnetical investigation.

Believe me, J. H. L.

F. Ronalds, Esq. [Letter torn]

June 28. I have completed the adjustment . . . . morning.

Toronto, July 17, 1850.

My dear Sir,

I have no very brilliant success to report as yet. I have had several good traces, although none so strong as those you sent out, and several failures. The latter from two causes, defective cleaning and overcoating. The cleaning of such large plates appears to me to be a matter of much greater difficulty than in proportion to small ones, and my American adviser who is by far the most skilful operator I have met with, strongly recommends a complete change in the *modus operandi*, which he regards as *old fashioned* and inferior to the methods in use among the American artists. This step however I have not taken, except so far as to adopt their first process in place of *that detestable* oil and rotten stone. The time consumed in cleaning, coating, mercurializing, tabulating ordinates and tracing (which last however I have not yet done) will apparently run away with a large part of the working hours of every day. This however, I am aware, I am in no condition as yet to judge of quite fairly. My failures in coating were occasioned by the great heat of the weather which filled the batteries with gas and not having the right scale of tints, I overcoated. In most cases the passage from light straw to rose (over the iodine) was almost instantaneous, and the intermediate golden straw could not be detected. I also made the mistake of allowing too long over the iodine the second time.

Pure bromine water as an accelerator is not liked by the Americans, and I have found it to evaporate with such rapidity that I have already used more than one (about 1 and 1/2) of your eight boxes of it. At this rate the supply will scarcely last four months, more particularly as out of the 24 capsules per *Pearl*, one was broken, two empty, and the bromine of a fourth had escaped, leaving

only a colorless fluid. I have not examined the other batch. This is a loss of 1/6th. I should be obliged by your telling me now many times a capsule should renew. I have decanted them by the box-full at a time, into a well stoppered bottle, and find it takes about 1/2 or 2/3 the contents of one on each occasion. I am at present using a compromise accelerator such as I have previously used in practice (taking portraits) 2/3 oz. of chloride of iodine, 1/3 oz. bromine, 1/4 oz. chloride bromine to one pint of lime water, with a few drops (say 10) of muriatic acid in addition. The first trace of a plate thus prepared (yesterday) was the best yet, but then it was the best cleaned, and was coated by Mr. Palmer. It failed however with artificial light, perhaps because the lamp was not at the right level, a circumstance I forgot to look to.

July 19th. Another plate similarly prepared has failed under artificial light, although the effect of daylight was sufficiently strong. On this occasion I looked to the light, it appeared to me to be too yellow for a good photogenic effect, but was as I should have thought powerful enough for anything. I noticed that the difference in intensity between the focus of the slit, and the adjacent diffused light, was much less than when daylight is used.

Is it your object to coat lightly with express reference to the stronger contrast of light and shade obtained that way? Your coating colours are far short of those directed by American operators.

"Iodize to an incipient rose, coat over quick to a deep rose, then back over iodine 1/10th as long as at first: gives a clear strong bold deep impression." This again as elsewhere explained—"over iodine to a *dark orange* just bordering on a rose colour, then over the quick to a soft rosy red, back 1/10th."

It has occurred to me while writing that my daylight is possibly too strong, the camera being between 3 and 4 feet only from the window. This I will try at once. I have written to New York about the cost and facilities of manufacturing either gas or camphine on the premises. The gas company here demanded £30 a year independently of the cost of gas consumed, for laying down pipes for a distance of about 2/3 of a mile.

In one or two experimental traces by the Vertical Force there was scarcely any deviation from a right line. Although I have raised the centre of gravity, I think to the utmost. At present I am not working it, and do not intend to do so until the chemical and mechanical operations are reduced to tolerable certainty. It would only be wasting plates.

Believe me, in haste, very faithfully yours

F. Ronalds, Esq. J. H. Lefroy

(*To be continued*)

PLATE XVII

TOTAL ECLIPSE OF THE MOON, DECEMBER 18, 1945

Part of a series of photographs of the lunar eclipse obtained at the David Dunlap Observatory by Ruth J. Northcott and Gerald F. Longworth. These photographs were made with the 19-inch reflector on Eastman 103a-0 spectroscopic plates. The exposure in each case was about one tenth of a second. The approximate times of the exposures, from left to right, were: 19:48, 20:00, 20:11, 20:22, and 20:33 E.S.T. (Moon entered umbra 19:38, middle of eclipse 21:20 E.S.T.). Reduced to seven-eighths of original size.

NOTES AND QUERIES

Communications are invited, especially from amateurs. The Editor will try to secure answers to queries.

Lunar Eclipse of December 18, 1945 (with Plate XVII)

The sky was clear at the David Dunlap Observatory while this eclipse was in progress and several photographs were secured with the 19-inch Reflector. They are reproduced in Plate XVII and details of the observations are supplied in the inscription.

C. A. C.

Minima of Algol, Predicted and Observed

A subject of great interest to many users of the *Observer's Handbook,* as judged from the editor's mail, is the prediction of times of minima of Algol. Apart from checking the approximate times for the purpose of merely knowing when to expect Algol to be faint, the subject is of great theoretical interest.

Mr. D. W. Rosebrugh, of the A.A.V.S.O., has for a couple years made careful observations of the brightness of Algol, to check the predictions. In a recently received letter, circulated to a few people immediately concerned with the A.A.V.S.O. or with publishing ephemerides, Mr. Rosebrugh discusses his recent observations. From his letter we quote the following results, leaving a detailed discussion of the observations to a future paper.

Summarizing this letter and that of Nov. 6, 1945.

	Predicted time in Handbook	Observed	Difference in Minutes
November 6, 1945	1932	1934¼	Late 2¼
November 24	0026	0017	Early 9
November 26	2115	2111½	Early 3½
		Sum	Early 10¼
		Average	Early 3.41 mins.

Perhaps some of you may feel that the November 24 observation should be thrown out, as being obviously inconsistent with the others. I do not know.

On November 27, 1943, two years ago to the day, I wrote Mr. Campbell and Mr. Seely and on July 24, 1944, I wrote Dr. Frank S. Hogg summarizing my 1943 observations as follows:

Date	Predicted time in Handbook	Observed	Difference in Minutes
February 15, 1943	2038	2033	Early 5

March 7, 1943	2224	2228	Late 4
September 18, 1943	2145	2140	Early 5
October 11, 1943	2014	2033	Late 19
November 20, 1943	2338	2337½	Early ½
		Sum	Late 12½
		Average	Late 2½

Here again, some of you may feel that the October 11, 1943, observation should be thrown out. If it is thrown out and also that of November 24, 1945, the average of the six remaining observations is 1.29 minutes early, and I am sure that I am not "that good".

The predictions in the Handbook R.A.S.C. are based, see page 3, on a minimum at J.D. 2429,234.6859. It is now nearly 7 years later but I cannot with my crude methods of observing detect any definite errors in the predicted times.

It is my considered opinion that the times predicted in the Handbook, R.A.S.C., are as nearly correct as anything that I can set up after spending many hours observing scattered over two years of time.

F. S. H.

News from Holland

Dr. Helen Sawyer Hogg, who for a number of years has corresponded with Dr. Oosterhoff and Dr. Martin on the problems of variable stars in globular clusters, has recently received a letter from Dr. Oosterhoff. This contains the following sad news of friends in the Dutch East Indies:

Sterrewacht Te Leiden,
12 November, 1945.

Dear Miss Sawyer,

The news from the Lembang Observatory in the Dutch East Indies is not so good. A week ago we received the bad tidings that Martin, whom you certainly know from his work on ω Centauri, died some months ago and from de Sitter we have had no news at all, although we know that his wife and four children are safe in Batavia. It seems that the chances are small that he is still alive. It would mean a heavy loss for the Dutch astronomy.

Very much has happened in the far east and very many will not come back. My younger brother died in Japan as a prisoner of war.

At the moment I have no opportunity to work on globular clusters, as I have no material left. I would have to make a second trip to the U.S.A. in order to get new plates and I hope that this will be possible some time.

There have been some changes at the Leiden Obsrvatory. Hertzsprung

retired and is in Denmark now and Oort is his successor as director of the observatory. I am now in charge of the astrophysical department.

P. Th. Oosterhoff.

Lieutenant George Tidy, R.C.N.V.R., recently returned from three and a half years as a Japanese prisoner-of-war in the Indies, had not met Dr. A. de Sitter in prison, but learned of his previous transfer from a Java prison camp to a work party in Burma—a transfer from which usually only a small percentage survived.

F. S. H.

What Use Astronomy—and the Atomic Bomb

One of the most embarrassing questions which astronomers are frequently called upon to answer is, "What use is astronomy? Why bother to look at a star you can see only through a 100-inch telescope?"

The atomic bomb should provide an eternal answer to such queries. So-called "practical" men would certainly have found little to interest them in the experiments and theories which atomic physicists found so fascinating prior to about 1940. It is true that the cyclotron had valuable applications in therapeutics. But on the surface, much of the work would have seemed as useless as the discovery of a white dwarf star or the identification of molecules in interstellar space. Yet both physicist and astronomer were concerned with the investigation of the fine structure of matter, the physicist in his laboratory on the earth, the astronomer in his laboratory of the sky. And investigation of the fine structure of matter was directly responsible for the atomic bomb.

The essential identity of the two lines of research is tacitly recognized in President Truman's statement, "The force from which the sun draws its power has been loosed against those who brought war to the Far East".—From a paper on "Atomic Power in the Laboratory and in the Stars" by R. S. Richardson in *Publications of the Astronomical Society of the Pacific*, October 1945.

C. A. C.

Ultimate Hypotheses of Natural Phenomena

The growth and continual change in the hypotheses proposed to "explain" the varied phenomena of nature provide interesting chapters in the history of science. Our readers will be pleased to read some references to this subject in the latest work of the author of the outstanding, and indeed definitive, life of Isaac Newton (1934).

Aristotle also endues the passive elements (earth, water, air, fire) with the active attributes of heat and cold, wet and dry. Thus, the essence of earth is cold and dry; of water, cold and wet; of air, hot and wet; of fire, hot and dry. And since the elements are varieties of universal matter, they are transmutable; earth to water by change of dry to wet, while the cold is not altered, etc.

Who can rightly assume that these ideas were without the support of careful observation? Is not the earth the solid core of our apparently stationary world, is it not covered by a shell of water, above that by one of air, and over all by the heavens with its pure fire gathered in the stars? And did not Aristotle have evidence to convince him that each of the elements sought its own kind and its own place as planned by the Creator, when he saw earth gravitate to the bottom and air levitate to the top of a vessel of impure water? It is of course true, that as knowledge slowly accumulated and observation was refined, the attempt to reconcile the increasing facts with the old formulae led to all manner of confusion, as it always did and does to-day. It took many centuries, and the slow invention of apparatus, before Copernicus could propose, and Galileo could convince, that the earth revolved about the sun. It took another century before Boyle could prove that the air was a body having weight, and Newton could discover the law of universal gravitation. Who knows to-day why inert matter attracts, or why electricity can both attract and repel? Certainly the profound attempt of Professor Einstein to reconcile that bewildering paradox by a mathematical formula has reduced the universe to a habitation such as dreams are made of. The only real difference one can find between the long stifling of the acquisition of knowledge by a rigid adherence to the hypothesis of Aristotle, and our present state is that knowledge accumulates far more rapidly than it did then; and, from sad experience and disappointed hopes, we discard our cherished hypotheses more readily. The mere discovery of X-rays in 1895 wrecked, in less than a quarter of a century, the hypotheses of light and electricity built up so laboriously during the nineteenth century. The day before that discovery was made, the physicist believed that his only business was to make his observations correct to the next decimal point; but since then, his hypotheses have been as evanescent as a phantasm.

* * * * *

Such was the fate of Aristotle's cosmic hypothesis; from a lack of contructive scepticism, it simply throttled new ideas; such also has been the effect

of all its successors. Newton's corpuscular hypothesis of the nature and cause of light had to be discarded. So, too, we may cite from other fields Darwin's hypothesis of natural selection; and the psychologist's hypothesis of materialism. It was for this reason that Newton excluded hypotheses from the field of science, and Poincaré warned scientists to regard them as mere verbal analogies to be indulged in as warily as possible.—From *The Life and Works of the Honourable Robert Boyle,* by L. T. More, pages 258, 261 (1944).

C. A. C.

MEETINGS OF THE SOCIETY

AT MONTREAL

March 22, 1945.—Dr. W. F. Oliver, of the Department of Physics, Macdonald College, addressed the Montreal Centre on "Relativity". Dealing with a subject essentially mathematical in nature, Dr. Oliver nevertheless succeeded admirably in making it intelligible to a largely non-mathematically minded audience, "for mathematics", he said, "is only a tool by means of which we arrive at certain results."

Before the turn of the century, space and time were regarded as absolute quantities. In fact, for ordinary terrestrial phenomena Newton's laws had worked out very well and belief in these classical ideas was so strong that they came to be accepted as necessary truths. Modern refined technique in experimental physics, however, led to contradictions and the classical conception of time and space had to be revised.

The discovery that the velocity of light in a vacuum represents the upper limit for all physical velocities was one of the most important experimental facts in upsetting the accepted beliefs. In explaining this to his audience Dr. Oliver borrowed the illustration from Gamow's "Mr. Tompkins in Wonderland" of the tramp racing, at three-quarters the velocity of light, along the cars of a train also travelling at three-quarters the velocity of light, proving by formula that he would never overtake the beam of light from the signal lamp.

In an equally graphic manner, Dr. Oliver explained "shortening of space" and "dilation of time", showing that measurement depends upon what we are doing at the time of making the measurement and that simultaneousness has an exact meaning only if events are viewed from the same system of reference. To check these revolutionary ideas we turn to atomic physics where electrons reaching a velocity of .998 that of light provide the speed we need to make the test.

Curvature of space was a more difficult idea to convey without entering the realm of higher mathematics, but Dr. Oliver again reminded his listeners that our tool, mathematics, will do it easily. While many of us, it is true, cannot use this tool, nor have we words to describe the mathematical processes, we can see the results. "Einstein", he said in conclusion, "has opened up a whole new realm of thought, getting our minds out of a rut and taking us from our earth to a much grander universe."

April 26, 1945.—The Reverend Frère Robert addressed the Montreal Centre in the French language, choosing as his subject "L'astronomie à l'école des grands écrivains".

Everyone should study astronomy to some extent, Frère Robert said in introduction. The universities do not teach this subject with the object of

making astronomers of the students but to give them some understanding of daily phenomena. If we examine the works of the great writers we find many obvious errors from the astronomer's point of view. As Frère Robert emphasized, though, in proving that they were not great astronomers it was not his intention or desire to detract in any way from their greatness as writers.

The speaker first selected passages from the writings of Chateaubriand. This author had in a number of instances left himself open to correction by mentioning the phase of the moon in connection with some particular date or event. For instance, he refers to the moon on 16th November 1841, whereas it can be calculated that on that date it was new moon and, of course, invisible.

Alfred de Musset in his poem "La ballade de la lune" gives a beautiful description of the moonlight shining on the belfry but later speaks of returning at the same hour each night to watch this lovely effect, thus ignoring the continual changes in the phases of the moon and and the times of moonrise and moonset.

Other authors, including Jean Narrache, Victor Hugo and Henri de Régnier, have made similar mistakes. The most common error, it seems, is with regard to the position of the crescent moon, for the writers overlook the fact that the illuminated side of our satellite must always be that nearest the sun, whether at sunrise or sunset, and more than one author has been guilty of describing Venus, the "evening star", as rising in the east while the sun sinks below the western horizon.

In pointing out the error in each astronomical reference, Frère Robert combined his explanation with additional helpful information, such as how to calculate the date of Easter and how to tell the time by the Big Dipper.

Frère Robert, who received his doctorate of science from the University of Lille, France, is at present professor of mathematics and astronomy at Mont St. Louis College. He is also author of "L'astronomie élémentaire", the second edition of which was published this year.

May 19, 1945; *Star Night.*—On the grounds of Lower Canada College, the Montreal Centre held its first public "Star Night"—the most ambitious undertaking of the Centre to date. Well advertised by newspaper and radio, the event was planned originally for Wednesday but due to unfavourable weather conditions had to be postponed each evening until Saturday when the sky finally cleared. This postponement undoubtedly affected the attendance but nevertheless several hundred people arrived on the Saturday from all parts of the city.

Long before dark the crowds began to gather and watched with interest while the telescopes, nine in number, were being assembled and set up in position. These included three reflectors and six refractors. The reflectors, two 5-inch and one 8-inch mirrors, were made by their owners, F. W. Henshaw, J Naubert and Jacques Labrecque. The refractors ranged from a portable Zeiss to a 4-inch Zeiss and a 4-inch "Lancaster". The Centre's 6-inch

refractor could not be transported to the college grounds, being permanently mounted now in the Ville Marie Observatory. Judging by the questions and comments, the different types of instruments and mountings aroused almost as much interest as the celestial objects viewed.

The program began officially at nine o'clock when Daniel P. Gillmor, President of the Montreal Centre, addressed a few words of welcome to the visitors. The microphone was then handed over to Mr. F. DeKinder who gave a short talk on the Moon, Jupiter and Saturn, while long queues of spectators formed at each telescope to view these objects. The Moon, considerably older and brighter than on the original date, tried to steal the show, but the two planets came in for their fair share of attention. Later, as the sky darkened, Mr. DeKinder proceeded to point out the main constellations, telling many interesting facts and legends about each.

Arrangements for "Star Night" were under the direction of DeLisle Garneau, Director of Observations of the Montreal Centre, assisted by his committee. Programs handed out at the gate to each visitor, gave information regarding the objects to be viewed. Separate enclosures, roped off for each telescope and bearing signs indicating the object on which the telescope was turned, facilitated the handling of the crowds, while the public address system, which carried Mr. DeKinder's talk on the constellations to all corners of the grounds, kept the people from becoming restless while waiting their turn at the telescopes.

"Star Night", the first event of its kind to be held in Montreal, was in the nature of an experiment, but judging by its success it is evident that it appealed to the public of Montreal, and the Montreal Centre will probably make it an annual event.

ISABEL K. WILLIAMSON,
Recording Secretary.

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